POLICY RESEARCH WORKING PAPER 1945

# Evaluating a Targeted Social Program When Placement Is Decentralized

*Martin Ravallion*
*Quentin Wodon*

A social program that relies partly on geographic decentralization for placement provides indicators helpful for identifying the program's impact on welfare.

The World Bank
Development Research Group
July 1998

## Summary findings

An assessment of the welfare gains from a targeted social program can be seriously biased unless it takes proper account of the endogeneity of program participation.

Bias comes from two sources of placement endogeneity: the purposive targeting of the geographic areas to receive the program, and the targeting of individual recipients within selected areas.

Decentralization of program placement decisions is common, because of the administrative cost of centralized placement decisions and the fact that local groups and governments are likely to be better informed about who most needs help. But full decentralization is uncommon; the center typically retains control of broad geographic targeting.

Ravallion and Wodon argue that partial decentralization of program placement decisions creates control and instrumental variables useful for identifying program benefits.

The central allocation to a local level of government is presumably based on observable indicators. The central allocation will also influence the allocation to an individual but is unlikely to determine outcomes at the individual level conditional on individual program participation. So with suitable controls for the welfare-relevant geographic characteristics determining program placement decisions, the center's allocation across areas can be used as an instrumental variable for individual participation.

The authors use Bangladesh's Food for Education program to illustrate their approach. A single post-intervention cross-sectional household survey was used to identify the impact of the program on school attendance, using geographic placement at the village level as an instrument for individual program placement. To deal with bias from the endogeneity of village selection, the authors used a detailed community survey coordinated with the household survey to control for likely sources of heterogeneity in geographic influences on school attendance, consistent with prior information on how the government targeted the program geographically.

They found that the programs had significant and sizable impacts on school attendance. At mean points, the program's incentive increased attendance by 24 percent of the maximum feasible days of schooling.

A regression estimator ignoring the purposive program placement was found to result in a substantial underestimation of the program's impact. Indeed, the simplest possible control group method—assuming that nonparticipants provide a valid counterfactual — performed much better than a regression method treating placement as exogenous.

---

This paper — a product of the Development Research Group — is part of a larger effort in the group to evaluate the impact of social programs. The study was funded by the Bank's Research Support Budget under the research project "Policies for Poor Areas" (RPO 681-39). Copies of this paper are available free from the World Bank, 1818 H Street NW, Washington, DC 20433. Please contact Patricia Sader, room MC3-632, telephone 202-473-3902, fax 202-522-1153, Internet address psader@worldbank.org. Martin Ravallion may be contacted at mravallion@worldbank.org. July 1998. (17 pages)

# Evaluating a Targeted Social Program When Placement is Decentralized

Martin Ravallion and Quentin Wodon[1]

[1] Ravallion is with the Development Research Group, World Bank, Washington DC. Wodon is with the Development Research Group, and the University of Namur (FUNDP), Belgium. This study was made possible through a long term collaborative effort between the Bangladesh Bureau of Statistics and the Poverty Reduction and Economic Management Unit of the World Bank's South Asia Region. The paper was prepared as an input to World Bank (1998). The support of the World Bank's Research Committee (under RPO 681-39) is gratefully acknowledged, as are the comments of Harold Alderman, Jyotsna Jalan, Manny Jimenez, Shekhar Shah, Jonathan Morduch, K. Subbarao and Dominique van de Walle.

## 1. Introduction

It is known that an assessment of the welfare gains from a targeted social program can be seriously biased unless it takes proper account of the endogeneity of program participation. Two sources of bias can be identified. First there is placement endogeneity due to purposive targeting of the geographic areas which are to receive the program. Secondly, there is placement endogeneity due to targeting of the individual recipients within the selected areas.

This paper explores the scope for identifying the micro-level welfare impact of a social program which relies in part on geographic decentralization for its placement. Decentralization is often favored precisely because it can exploit specialized local knowledge for targeting – knowledge that is not available to the center. For example, local community groups might prepare lists of beneficiaries based on their own perceptions of need. Because this information is unobserved by the center (which is arguably the main reason why decentralization was favored in the first place) it is implausible that one could ever find suitable control variables to deal with endogeneity of individual placement. As is well recognized in the evaluation literature,[2] to deal with this type of problem one needs an instrumental variable which determines program placement at the individual level without also determining program outcomes conditional on placement. At first glance, one might well be skeptical of ever finding such a variable. The local community group can be assumed to target the program according to a set of observed household characteristics, every one of which would presumably also influence the household's behavior and welfare, and thus should appear in a model for any likely outcome indicator.

---

[2] See, for example, Heckman and Robb (1985) and Mofitt (1991).

However, decentralization in this context often entails that the central government first allocates across a lower level of government (defined geographically) and then governments at that level allocate to a lower level and so on. We call this "partial decentralization". A feature of partial decentralization is geographic separability, whereby the allocation across individuals within a given area is conditional on the allocation to that area, and is otherwise independent of the attributes of other areas. Many targeted public programs have a placement structure of this sort. For example, school-based food distribution programs typically involve allocation decisions first at the school or area level, and then among children within each school or area.

We argue that this common feature of decentralized programs helps the evaluation in two ways: Firstly, the fact that the center retains control of the geographic placement suggests that suitable control variables should be observable to deal with this source of endogeneity. In World Bank poverty projects, for example, considerable attention is typically given to geographic placement on the basis of the geographic poverty profile. Then, in principle, one should be able to find suitable control variables for geographic placement, and thus treat this aspect of the problem as "selection on observables" (Barnow et al., 1980; Heckman and Robb, 1985). There will no doubt be some omitted variables in any empirical model of geographic targeting, but with information on the program and geographic data, this problem should be limited.

Secondly, partial decentralization can help by creating a valid instrumental variable for individual program placement. With geographically decentralized placement, and suitable controls for household and geographic heterogeneity, we argue that program impacts at the individual level can be estimated in a believable way while allowing for the endogeneity of both geographic and individual placement.

We apply the method to Bangladesh's Food for Education (FFE) program. The program aims to keep the children of poor rural families in school. Participating households receive monthly rations of food as long as they send their children to primary school regularly. Targeting is done in two stages; first local areas are chosen by higher levels of government, and then individual participants are selected by local community groups exploiting idiosyncratic information.

The following section presents our model and estimation method. Section 3 applies the method to Bangladesh's FFE program. Section 4 concludes.

## 2. Program Placement Model and Evaluation Method

A social program allocates $IP_i$ (for "individual placement") to the $i$'th individual. The individual welfare outcome is $W_i$ which is assumed to depend linearly on $IP_i$ as well as vectors of household characteristics $\boldsymbol{X}_i$, and geographic characteristics $\boldsymbol{Z}_i$. The regression model for the welfare outcome is:

$$W_i = \alpha IP_i + \boldsymbol{\beta}'\boldsymbol{X}_i + \boldsymbol{\eta}'\boldsymbol{Z}_i + \mu_i \qquad (1)$$

where $\boldsymbol{X}$ and $\boldsymbol{Z}$ are assumed to be exogenous (orthogonal to $\mu$) but $IP$ is not.

Equation (1) is a reasonably standard formulation in the evaluation literature, though it has limitations. Linearity in $IP$ entails that the program has the same marginal impact for everyone. One can readily relax this by allowing interaction effects with $\boldsymbol{X}$ and/or $\boldsymbol{Z}$. However, consistent estimation allowing for idiosyncratic impacts on outcomes at given $\boldsymbol{X}$ and $\boldsymbol{Z}$ is not possible unless the idiosyncratic factors do not influence program placement (Heckman, 1997).

How is *IP* determined? We might imagine that the central government directly chooses which individuals are allowed to participate. However, this is not a realistic model of public decision making, since it assumes too much about the information available to the center. More plausibly, the center leaves local governments or Non-Governmental Organizations (NGOs) in each area to determine the allocation across individuals. This is administratively easier, and also takes advantage of the fact that lower levels of government or local community organizations are presumably better informed.

An implication of such decentralization of program placement is that the allocation to the $i$'th household will depend on whether or not the program has been placed in its area of residence, which we denote by the variable $GP_i$ (for "geographic placement"). The allocation will presumably also depend on household characteristics, not all of which are observed. We write the model determining individual placement as:

$$IP_i = \gamma GP_i + \boldsymbol{\pi}'\boldsymbol{X}_i + \nu_i \qquad (2)$$

where $\nu$ is an error term embodying the unobserved influences on *IP*. The endogeneity of program placement at the household level means that the error term in equation (2) is correlated with that in (1). If the program is targeted to households with low (high) values of the outcome indicator then there will be underestimation (overestimation) of the program's impact. To obtain consistent estimates of program effects with a single cross-section survey we can, however, use *GP* as an instrument for *IP* as long as *GP* is not itself correlated with $\mu$. For this condition to hold it is crucial that the vector $\boldsymbol{Z}$ contains all welfare-relevant variables used by the center in deciding geographic placement. If the omitted variables which determine the center's choice of target areas also alter outcomes at the household level then *GP* will no longer be a valid instrument for *IP*. Whether the assumption that ***Z*** contains all relevant control variables is defensible in practice will depend on how much infomation the evaluator has on how the center chooses program areas.

This approach does not require a baseline survey. For the class of partially decentralized programs described above, consistent estimates of the welfare impacts are possible using a single cross-section survey. The cross-sectional data must, however, include both household characteristics and relevant characteristics of the geographic area in which the household lives. It is not uncommon for household socio-economic surveys to include surveys of the infrastructure and services available in the area of residence for each sampled household. In the application which follows we have a community survey which includes a wide range of geographic variables of likely relevance to the center's geographic placement decisions; in this case there is less concern about omitted geographic variables than there would be without such data.

Notice that if one had not used geographic data on program placement as an instrument for individual placement, then one could deal with any omitted geographic variables by including a complete set of geographic dummy variables (or, equivalently, taking deviations from geographic means). However, that option is precluded here, since the geographic dummy variables will be collinear with *GP*.

So there is a sense in which dealing with one source of bias in the outcome equation, namely placement endogeneity at the individual level, limits our ability to deal with another source of bias, namely omitted geographic variables determining both outcomes and program placement by geographic area.

However, it can be argued that when the center is trying to assess the impact of a decentralized program, the greater concern must be unobserved determinants of individual placement by lower levels of government or NGOs. The point of decentralization is to exploit local information not available to the center. By contrast, the center's geographic targeting of program areas must presumably be based on variables which are observable to the center.

## 3. Bangladesh's Food for Education Program

FFE was launched on a pilot basis in July 1993 and has grown since then into a major national program. Its objectives are to increase primary school attendances for poor children by providing rations of rice or wheat to selected households as an incentive to parents. The total budgetary cost from July 1993 to June 1997 was Tk 760 crores or $175 million (BIDS, 1997: 8). The program's share of the budget of the Primary and Mass Education Division increased from 11 percent in 1993-94 to 26 percent in 1995-96. In 1995-96, 2.2 million children, or about 13 percent of the total enrollment in mainstream schools–participated in the program.

The program has a hierarchical targeting structure. Bangladesh's administrative structure consists of (in decreasing order by size) divisions, districts, thanas, and unions. The program covers all thanas, and one or more unions are picked in each thana. The program stipulates that these should be economically backward unions, and unions with low schooling attainments. Within the selected unions, FFE is granted to all primary schools.

Second, within the selected union, targeting is done at the household level. Community groups select beneficiaries and distribute the food. The program rules suggest of criteria for targeting (landless households, female-headed households, and households whose parents work in low-income professions). However, there is clearly a degree of discretion in individual targeting.

If a household is selected to participate in FFE, it is entitled to 15 kg of wheat or rice per month for one child going to school, or 30 kg if the household has more than two children and all of them attend school regularly. To receive their rations, the enrolled children must attend at least 85 percent of the classes each month. By the third day of each month, the headmaster of the school establishes the list of all students from beneficiary households who met the 85% attendance threshold for the previous month. The total rations needed are then estimated and submitted to the thana executive for approval. The required food is made available by the thana to the school with an additional allowance to cover the costs of transport, distribution, and handling. The distribution is made each week.

Our data come from the 1995-96 nationally representative Household Expenditure Survey (HES) of the Bangladesh Bureau of Statistics. The HES included questions on FFE participation and also had a community survey done at the local level.

To compute the attendance rate, we took into account the number of school days missed by each student, as well as the number of days during which the school was closed and he or she could not attend. A child not enrolled was given a zero attendance rate (thereby, we capture enrollment and attendance with one measure). For those enrolled, the attendance rate was calculated as the ratio of the actual attendance to maximum feasible attendance, given that there are 235 school days per year in Bangladesh.[3] The outcome variable $W$ in equation (1) is the

[3] The HES gives for each child the school days missed and the number of days that the school was closed. Actual attendance was estimated as 235 days minus school days missed minus school days

mean attendance rate for each household. The sample mean attendance rate was 62.5%. For households participating in the program (about one tenth of the sample), the attendance rate was 79.8%, versus 60.3% for non-participants.

The measure of household participation is the quantity of foodgrains received under the program. The mean amount received by the participating households was 114 kg per year. The community module provides independent information on whether the community also participates in FFE. Our measure of geographic placement (*GP*) takes the value one if the community survey indicates that the community participates.

The regressors in the vector $\boldsymbol{X}_i$ in equation (1) included household size variables, family structure variables, the education levels of the father and the mother,[4] the level of land ownership in the household, the age of the child and its square, the religion of the household, and whether or not the household receives FFE.

The geographic variables comprise two sets. The first are those we identified as likely explanatory variables in a model such as this, even if there was no concern about endogeneity of geographic placement. These comprised distances to school, the type of school (governmental, private, NGO), and a series of school quality variables reported in the community survey. The second set of geographic variables are controls to deal with endogenous geographic placement. The indicators used for this purpose included land distribution, irrigation intensity, road quality, electrification, distance and time to thana and district headquarters and to the capital (Dhaka),

---

closed, while maximum feasible attendance was 235 minus school days closed.

[4] The excluded dummies are illiterate father and mother. In Table 1, class 1 to 5 represents some primary education, class 5 primary education completed, class 6 to 9 some secondary education, and higher level secondary education or above (e.g. professional or university degree) completed.

distance to various facilities (health care, Banks, government agencies), incidence of natural disasters, attitudes to womens' employment, education and family planning, average schooling levels of the head and spouse, majority religion of the village, and population size of the village.

These geographic variables were (jointly) good predictors of program placement. A probit regression of whether the village had the program on a range of likely indicators of "economic backwardness" from the community survey gave a pseudo-$R^2$ of 0.55 (Chi-square of 91.7 which is significant at the 0.5% level, with 166 observations).

## 4. Results

We estimated the system made up of equations (1) and (2) by Three Stage Least Squares (3SLS), and compared it to the Ordinary Least Squares (OLS) estimate of equation (1). Table 1 gives the OLS estimate of (1) and the 3SLS estimate of (1) and (2). We give two versions of the OLS equation; the first excludes the control variables for endogeneity of geographic placement, while the second does not.

The regression for the average attendance rate looks sensible on the whole. Larger families, and with higher proportions of young children, tend to have a lower average attendance rate, suggesting crowding out. Higher education levels of both the mother and father increase school attendance rates of children. So does greater wealth, as measured by land ownership. However, it is unclear why proximity to a boys' school raises participation, while being closer to a girls' school has the opposite effect.

Turning to the FFE program, the participation by the community (the instrumental variable needed for identification) is a highly significant determinant of individual participation.

The estimated parameter for program participation ($\alpha$) in the school attendance equation is positive and significantly different from zero for both the OLS and 3SLS methods, the latter allowing for endogeneity of placement at both community and household level. However, the 3SLS estimate of $\alpha$ is 66% higher than the OLS estimate without the geographic controls, and 49% higher than the estimate with those controls. Almost all of the difference between the 3SLS estimate and the simple OLS estimate is due to allowing for endogeneity of placement at the individual level.

The coefficient $\alpha$ directly gives the increase in the attendance rate attributed to an extra 100 kilos of foodgrain. Controlling for other characteristics, 100 kilos of grain increases the attendance rate by 0.21 when one uses the 3SLS estimate, versus 0.13 using the simple OLS model (and 0.14 for the model with geographic controls). When compared to the counter-factual of obtaining nothing from the program, receiving the average amount (114 kg of grain) raises the attendance rate by 0.24, i.e. an extra 24% of the maximum feasible days of school attendance can be attributed to the incentive provided by the program.[5]

On a priori grounds, the OLS estimate seems implausibly low. The mean attendance rate of program participants is 84% (very close to the stipulated attendance rate of 85%, though clearly this is not policed rigorously). The mean for participants is 60%. So our 3SLS estimate at mean FFE allocation turns out to be the difference between these two means. We would get the same estimate of program impact if we simply made the naive assumption that non-participants are a valid control group. However, the OLS estimate at the mean implies an attendance rate of

[5] The implications of this finding for an overall assessment of the cost effectiveness of the program are examined in Wodon (1998).

70% for FFE participants, which is well above the mean for non-participants, implying perverse targeting of the program to kids with above average attendance.

We cannot of course rule out the possibility of remaining bias in our estimate of program impact, due to some omitted determinant of geographic placement correlated with individual attendances. Adding our geographic controls did help reduce the obvious bias in our OLS estimate. Possibly if we had longitudinal geographic data then we could reduce the OLS bias further. But in our analysis the bulk of the work is being done by the treatment for endogeneity in individual targeting. In the context of decentralized programs such as FFE in Bangladesh, this is arguably the bigger problem.

## 5. Conclusion

Decentralization of program placement decisions appears to be common, and understandably so, given the potentially high administrative cost of centralized placement decisions across all individuals, and the fact that local governments and community groups are likely to be better informed about who is most in need of help from the program. However, full decentralization appears to be uncommon; more typically, the center retains control of the broad targeting across geographic areas.

We have argued that partial decentralization of program placement decisions creates useful control and instrumental variables for identifying program benefits. The central allocation to the relevant local level of government is assumed to be based on observable indicators. The central allocation will also influence the allocation to an individual, yet is unlikely to be a determinant of outcomes at the individual level conditional on individual program participation.

So, with suitable controls for the welfare-relevant geographic characteristics determining the center's program placement decisions, one can use the center's allocation across areas as an instrumental variable for individual participation.

The level of aggregation is important to our approach. If one were to aggregate up to the level of local government areas for assessing welfare gains when there is only one level of government then one would lose identification. In a federal system with multiple layers between the center and the individual program participant one can still aggregate geographically as long as there is one layer left for identification with sufficient variation in placement horizontally across that level. In the seemingly common case of having geographically-clustered household level data – with the clusters mapping into local levels of government with decision-making power over the allocation of a centrally instigated program – we are able to identify welfare impacts at the micro level.

For our method to work well with only cross-sectional data, it is important that the researcher can find sufficient control variables for geographic heterogeneity. As is invariably the case without longitudinal observations, latent effects due to omitted variables correlated with program placement can bias the estimates. Of particular concern in this context is any omitted geographic heterogeneity which jointly influences outcomes and the geographic placement of programs, since this undermines our case for using area placement as an instrument for individual placement. This underlines the importance of combining the household survey data with a geographic data base. Many surveys are now doing this. It also points to the importance of assuring that the geographic data collected correspond closely to the data actually used by central program administrators.

We have used Bangladesh's Food for Education program to illustrate the approach. A single post-intervention cross-sectional household survey was used to identify the impact of the program on school attendances, using geographic placement at village level as an instrument for individual program participation. To deal with bias due to endogeneity of village selection, we used a very detailed community survey coordinated with the household survey to control for likely sources heterogeneity in geographic influences on school attendance, consistent with prior information on how the government targeted the program geographically.

We found significant and sizable impacts of the program on school attendance. At mean points, the program's incentive increased attendance by 24% of the maximum feasible days of schooling. A regression estimator ignoring the purposive program placement was found to result in a substantial underestimation of the program's impact; indeed, the simplest possible control group method – assuming that non-participants provide a valid counter-factual – performed much better than a regression method treating placement as exogenous.

## References

Bangladesh Institute of Development Studies, 1997, An Evaluation of the Food for Education Program: Enhancing Accessibility to and Retention in Primary Education for the Rural Poor in Bangladesh, Mimeograph, Dhaka.

Barnow, B., G. Cain and A. Goldberger, 1980, Issues in the Analysis of Selectivity Bias, in E. Stromsdorfer and G. Farkas (eds) Evaluation Studies Review Annual, Vol. 5: 42-59.

Heckman, James, 1996, Randomization as an Instrumental Variable. Review of Economics and Statistics, Vol. 77(2): 336-41.

____________, 1997, Instrumental Variables. A Study of Implicit Behavioral Assumptions Used in Making Program Evaluations. Journal of Human Resources, Vol. 32(3): 441-461.

Heckman, James and Richard Robb, 1985, Alternative Methods of Evaluating the Impact of Interventions. In J. Heckman and B. Singer, eds, Longitudinal Analysis of Labor Market Data, New York: Wiley.

Mofitt, Robert, 1991, Program Evaluation with Nonexperimental Data, Evaluation Review, Vol. 15(3): 291-314.

Wodon, Quentin, 1998, Cost-Effectiveness of Food for Education in Bangladesh, Background Paper for World Bank (1998).

World Bank, 1998, Bangladesh: From Counting the Poor to Making the Poor Count. Report 17534-BD, Poverty Reduction and Economic Management Network, South Asia Region, World Bank.

**Table 1: School atendance and FFE program participation, Bangladesh 1995-96**

| | School attendance | | | | | | | | | | | | Program participation 3SLS | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | OLS without geographic controls | | | OLS with geographic controls | | | 3SLS | | | | | | | | |
| | coef. | | s.e. | coef. | | s.e. | coef. | | s.e. | | | | coef. | | s.e. |
| Constant | -0.681 | * | 0.102 | -0.582 | + | 0.298 | -0.600 | * | 0.293 | | | | 0.045 | | 0.309 |
| **Demographics** | | | | | | | | | | | | | | | |
| Log household size | -0.084 | * | 0.021 | -0.060 | * | 0.023 | -0.060 | * | 0.023 | | | | 0.011 | | 0.024 |
| Share boys 5 to 9 | -0.294 | * | 0.075 | -0.247 | * | 0.083 | -0.248 | * | 0.082 | | | | 0.018 | | 0.086 |
| Share girls 5 to 9 | -0.279 | * | 0.077 | -0.222 | * | 0.086 | -0.236 | * | 0.085 | | | | 0.186 | * | 0.089 |
| Share boys 10 to 16 | -0.234 | * | 0.070 | -0.178 | * | 0.078 | -0.179 | * | 0.076 | | | | 0.049 | | 0.081 |
| Share girls 10 to 16 | -0.111 | | 0.073 | -0.088 | | 0.080 | -0.088 | | 0.079 | | | | 0.062 | | 0.083 |
| Share adults male 17 to 40 | 0.123 | | 0.077 | 0.070 | | 0.086 | 0.069 | | 0.085 | | | | -0.022 | | 0.090 |
| Share adults female 17 to 40 | -0.035 | | 0.089 | 0.077 | | 0.099 | 0.076 | | 0.098 | | | | -0.034 | | 0.103 |
| Share adults male above 40 | -0.023 | | 0.090 | -0.030 | | 0.100 | -0.046 | | 0.099 | | | | 0.221 | * | 0.104 |
| Share adults female above 40 | 0.002 | | 0.089 | 0.027 | | 0.099 | 0.035 | | 0.097 | | | | -0.079 | | 0.102 |
| Female household head | 0.013 | | 0.047 | 0.006 | | 0.050 | 0.002 | | 0.050 | | | | 0.057 | | 0.052 |
| No spouse, married | -0.011 | | 0.043 | -0.025 | | 0.047 | -0.019 | | 0.046 | | | | -0.074 | | 0.049 |
| No spouse, single | -0.013 | | 0.039 | 0.007 | | 0.043 | 0.006 | | 0.042 | | | | 0.031 | | 0.044 |
| No spouse, div./widowed | -0.092 | * | 0.046 | -0.056 | | 0.049 | -0.053 | | 0.048 | | | | -0.075 | | 0.051 |
| **Education father** | | | | | | | | | | | | | | | |
| Below class 5 | 0.090 | * | 0.019 | 0.080 | * | 0.022 | 0.078 | * | 0.022 | | | | 0.027 | | 0.023 |
| Class 5 | 0.137 | * | 0.024 | 0.097 | * | 0.027 | 0.099 | * | 0.027 | | | | -0.045 | | 0.028 |
| Class 6 to 9 | 0.160 | * | 0.021 | 0.134 | * | 0.024 | 0.136 | * | 0.024 | | | | -0.006 | | 0.025 |
| Higher level | 0.173 | * | 0.031 | 0.163 | * | 0.035 | 0.166 | * | 0.035 | | | | -0.031 | | 0.036 |
| **Education mother** | | | | | | | | | | | | | | | |
| Below class 5 | 0.092 | * | 0.022 | 0.088 | * | 0.024 | 0.084 | * | 0.024 | | | | 0.046 | + | 0.025 |
| Class 5 | 0.066 | * | 0.024 | 0.055 | + | 0.029 | 0.053 | + | 0.029 | | | | 0.025 | | 0.030 |
| Class 6 to 9 | 0.072 | * | 0.030 | 0.059 | + | 0.034 | 0.059 | + | 0.034 | | | | -0.002 | | 0.035 |
| Higher level | 0.179 | * | 0.071 | 0.151 | + | 0.079 | 0.150 | + | 0.077 | | | | 0.026 | | 0.082 |

| | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Land ownership** | | | | | | | | | | | | |
| 0.05 to 0.49 acres | 0.050 | * | 0.018 | 0.030 | | 0.021 | 0.031 | | 0.020 | -0.002 | | 0.022 |
| 0.50 to 1.49 avres | 0.107 | * | 0.020 | 0.096 | * | 0.023 | 0.098 | * | 0.023 | -0.006 | | 0.024 |
| 1.50 to 2.49 acres | 0.096 | * | 0.025 | 0.098 | * | 0.028 | 0.104 | * | 0.028 | -0.074 | * | 0.029 |
| 2.50 acres or more | 0.071 | * | 0.023 | 0.079 | * | 0.028 | 0.084 | * | 0.027 | -0.073 | * | 0.029 |
| **Other household variables** | | | | | | | | | | | | |
| Mean age of the kids | 0.321 | * | 0.018 | 0.310 | * | 0.020 | 0.303 | * | 0.020 | 0.079 | * | 0.021 |
| Mean age of the kids squared | -0.018 | * | 0.001 | -0.017 | * | 0.001 | -0.017 | * | 0.001 | -0.004 | * | 0.001 |
| Non-Muslim | -0.059 | * | 0.020 | -0.036 | | 0.025 | -0.036 | | 0.025 | -0.006 | | 0.026 |
| **School variables** | | | | | | | | | | | | |
| Distance to primary school for boys | -0.001 | | 0.003 | -0.107 | * | 0.024 | -0.102 | * | 0.024 | -0.064 | * | 0.025 |
| Distance to primary school for girls | 0.001 | | 0.003 | 0.103 | * | 0.024 | 0.098 | * | 0.024 | 0.065 | * | 0.025 |
| Main school government aided | 0.007 | | 0.016 | -0.006 | | 0.021 | -0.006 | | 0.021 | 0.039 | + | 0.022 |
| Main school private | 0.107 | * | 0.035 | 0.055 | | 0.048 | 0.063 | | 0.047 | -0.122 | * | 0.049 |
| Main school NGO | -0.067 | | 0.075 | 0.014 | | 0.083 | 0.023 | | 0.082 | -0.005 | | 0.087 |
| **Complaints on government schools** | | | | | | | | | | | | |
| Not enough primary institutions | -0.003 | | 0.021 | -0.012 | | 0.030 | -0.008 | | 0.030 | 0.004 | | 0.031 |
| Not enough primary institutions for girls | -0.025 | | 0.029 | 0.012 | | 0.036 | -0.007 | | 0.036 | 0.273 | * | 0.037 |
| Not enough secondary institutions | 0.015 | | 0.024 | -0.030 | | 0.033 | -0.039 | | 0.032 | 0.082 | * | 0.034 |
| Not enough secondary institutions for girls | -0.034 | | 0.028 | -0.078 | + | 0.040 | -0.085 | * | 0.039 | 0.033 | | 0.041 |
| Insufficient quality of teaching | -0.059 | * | 0.026 | -0.040 | | 0.033 | -0.048 | | 0.033 | 0.109 | * | 0.034 |
| Insufficient number of teachers | -0.064 | * | 0.026 | -0.011 | | 0.036 | -0.019 | | 0.035 | 0.103 | * | 0.037 |
| Other complaints | 0.048 | + | 0.028 | 0.017 | | 0.040 | 0.008 | | 0.040 | 0.043 | | 0.042 |
| **Program variables** | | | | | | | | | | | | |
| Quantity of grain received by household | 0.127 | * | 0.015 | 0.142 | * | 0.018 | 0.211 | * | 0.039 | | | |
| Participation of village | | | | | | | | | | 0.506 | * | 0.021 |

Source: Source: Regressions by the authors using 1995-96 HES. Sample size: 3625 rural households. Adjusted R2 of 0.28 for OLS without geographic controls, 0.30 with geographic controls, 0.32 for 3SLS outcome equation, and 0.37 for 3SLS participation equation. Excluded categories for dummy variables are male household head, spouse present, illiterate father, illiterate mother, landless household, and Muslim household. See text for the variables used as geographic controls (50 community variables not shown in the Table). The symbols * and + denote significance at the 5 and 10 percent levels.

## Policy Research Working Paper Series

| | Title | Author | Date | Contact for paper |
|---|---|---|---|---|
| WPS1925 | Half a Century of Development Economics: A Review Based on the *Handbook of Development Economics* | Jean Waelbroeck | May 1998 | J. Sweeney 31021 |
| WPS1926 | Do Budgets Really Matter? Evidence from Public Spending on Education and Health in Uganda | Emmanuel Ablo<br>Ritva Reinikka | June 1998 | K. Rivera 34141 |
| WPS1927 | Revenue-productive Income Tax Structures and Tax Reforms in Emerging Market Economies: Evidence from Bulgaria | Fareed M. A. Hassan | June 1998 | A. Panton 85433 |
| WPS1928 | Combining Census and Survey Data to Study Spatial Dimensions of Poverty | Jesko Hentschel<br>Jean Olson Lanjouw<br>Peter Lanjouw<br>Javier Poggi | June 1998 | P. Lanjouw 34529 |
| WPS1929 | A Database of World Infrastructure Stocks, 1950–95 | David Canning | June 1998 | A. Abuzid 33348 |
| WPS1930 | The Main Determinants of Inflation in Albania | Ilker Domac<br>Carlos Elbrit | June 1998 | F. Lewis 82979 |
| WPS1931 | The Cost and Performance of Paid Agricultural Extenion Services: The Case of Agricultural Technology Transfer in Nicaragua | Ariel Dinar<br>Gabriel Keynan | June 1998 | F. Toppin 30450 |
| WPS1932 | Air Pollution and Health Effects: A Study of Respiratory Illness Among Children in Santiago, Chile | Bart D. Ostro<br>Gunnar S. Eskeland<br>Tarhan Feyzioglu<br>Jose Miguel Sanchez | June 1998 | C Bernardo 31148 |
| WPS1933 | The 1997 Pension Reform in Mexico | Gloria Grandolini<br>Luis Cerda | June 1998 | C. Zappala 87945 |
| WPS1934 | WTO Accession for Countries in Transition | Constantine Michalopoulos | June 1998 | L. Tabada 36896 |
| WPS1935 | Explaining the Increase in Inequality during the Transition | Branko Milanovic | June 1998 | G. Evans 85734 |
| WPS1936 | Determinants of Transient and Chronic Poverty: Evidence from Rural China | Jyotsna Jalan<br>Martin Ravallion | June 1998 | P. Sader 33902 |
| WPS1937 | Aid, the Incentive Regime, and Poverty Reduction | Craig Burnside<br>David Dollar | June 1998 | E. Khine 37471 |

## Policy Research Working Paper Series

| | Title | Author | Date | Contact for paper |
|---|---|---|---|---|
| WPS1938 | What Explains the Success or Failure of Structural Adjustment Programs? | David Dollar<br>Jakob Svensson | June 1998 | E. Khine<br>37471 |
| WPS1939 | Second Thoughts on Second Moments: Panel Evidence on Asset-Based Models of Currency Crises | Arturo J. Galindo<br>William F. Maloney | June 1998 | M. Cervantes<br>37794 |
| WPS1940 | The Structure of Labor Markets in Developing Countries: Time Series Evidence on Competing Views | William F. Maloney | June 1998 | M. Cervantes<br>37794 |
| WPS1941 | Are Labor Markets in Developing Countries Dualistic? | William F. Maloney | June 1998 | M. Cervantes<br>37794 |
| WPS1942 | Poverty Correlates and Indicator-Based Targeting in Eastern Europe and the Former Soviet Union | Christiaan Grootaert<br>Jeanine Braithwaite | July 1998 | G. Ochieng<br>31123 |
| WPS1943 | The Implications of Hyperbolic Discounting for Project Evaluation | Maureen Cropper<br>David Laibson | July 1998 | A. Maranon<br>39074 |
| WPS1944 | Detecting Price Links in the World Cotton Market | John Baffes | July 1998 | J. Baffes<br>81880 |